

See discussions, stats, and author profiles for this publication at: <https://www.researchgate.net/publication/231829911>

Ampère's Invention of Equilibrium Apparatus: A Response to Experimental Anomaly

Article in *The British Journal for the History of Science* · July 1987

DOI: 10.1017/S0007087400023979

CITATIONS

10

READS

736

1 author:



James R. Hofmann

California State University, Fullerton

30 PUBLICATIONS 135 CITATIONS

SEE PROFILE



The British Society for the History of Science

Ampère's Invention of Equilibrium Apparatus: A Response to Experimental Anomaly

Author(s): James R. Hofmann

Reviewed work(s):

Source: *The British Journal for the History of Science*, Vol. 20, No. 3 (Jul., 1987), pp. 309-341

Published by: [Cambridge University Press](#) on behalf of [The British Society for the History of Science](#)

Stable URL: <http://www.jstor.org/stable/4026360>

Accessed: 22/12/2012 18:40

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at
<http://www.jstor.org/page/info/about/policies/terms.jsp>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



Cambridge University Press and The British Society for the History of Science are collaborating with JSTOR to digitize, preserve and extend access to *The British Journal for the History of Science*.

<http://www.jstor.org>

Ampère's Invention of Equilibrium Apparatus: A Response to Experimental Anomaly

JAMES R. HOFMANN*

André-Marie Ampère's contributions to electrodynamics came at a late stage in an unconventional career. In 1820, he had reached the age of forty-five and had not as yet done any systematic research in physics. As a member of the mathematics section of the Académie des Sciences, his only significant contributions to the physical sciences had been some constructive criticisms of Fresnel's wave theory of light and three memoirs on chemical classification and gas theory.¹ Meanwhile, his longstanding interests in metaphysics and epistemology had resulted in pointed methodological and philosophical attitudes which both motivated and structured his subsequent work in electrodynamics. Not surprisingly, events during the first few months of Ampère's research included many unplanned and unexpected encounters as this wildly enthusiastic theoretician grappled for the first time with the recalcitrant complexity of actual experimentation in an uncharted new domain.

Historians have only recently begun to investigate this chaotic period in Ampère's career; in general, there has been an excessive reliance upon the famous expository memoir he published in 1826.² The preliminary pages of that rich blend of conceptual, mathematical and experimental argumentation have usually been glossed as Ampère's attempt to imitate Newton's creation of gravitation theory through a 'deduction from the phenomena'.³ This is, in fact, a fairly accurate rendition of the image Ampère wanted to broadcast in 1826; the general contour of this image should be clarified before its sources are scrutinized in more detail.

Two highly suggestive experimental discoveries had provided a lasting foundation for Ampère's efforts to create a new physics of 'electrodynamics'—the term he coined for the dynamics of moving electricity. In July 1820, Hans Oersted showed that an electric

1 For a good discussion of why Ampère became interested in electrodynamics, see K. Caneva, 'Ampère, the Etherians, and the Oersted Connection', *British Journal for the History of Science*, (1980), 13, pp. 121–138.

2 A-M. Ampère, *Théorie des Phénomènes Électro-dynamiques, Uniquement Déduite de l'Expérience*, Paris, November 1826.

3 For some examples, see P. Duhem, *The Aim and Structure of Physical Theory*, reprinted, New York, 1974; A. Kastler, 'Ampère et les lois de l'électrodynamique', *Revue d'Histoire des Sciences et Leurs Applications*, (1977), 30, pp. 145–157.

*History Department, California State University—Fullerton, California, U.S.A. 92634.

In spite of whatever flaws remain, the present form of this essay owes much to the helpful criticism of David Gooding, Rod Home and L. P. Williams.

current can change the natural orientation of a suspended bar magnet. During the following September and October, Ampère produced attractions and repulsions between wires conducting electric current; he attributed these phenomena to 'electrodynamic' forces and argued that these forces were also responsible for magnetic phenomena due to internal electric currents within all magnetized bodies.

For reasons I shall address in due course, Ampère assumed that an adequate theory of electrodynamic forces must include a force law expressing the attractive or repulsive forces between any two segments of an electric circuit small enough to be considered 'infinitesimal' in comparison to the distance separating them. By 1826, he had assembled an elegant and succinct derivation of his electrodynamic force law through the conceptual and mathematical analysis of four carefully chosen experimental demonstrations of states of equilibrium.

Each of these demonstrations used a balanced set of electrodynamic forces to produce a case of static equilibrium for a potentially mobile component of an electric circuit. Assuming that an electrodynamic force acts along the line joining any pair of circuit elements, Ampère applied the relevant laws of statics to provide a mathematical statement about the observed state of equilibrium in each case. Interpretations and applications of these equilibrium conditions in terms of infinitesimal current elements then provided the desired derivation of the electrodynamic force law as a function of the mutual orientation of any two elements and the distance between them. Such was the formal and polished process Ampère presented in his 1826 memoir as a model exercise in Newtonian methodology.

There has never been any doubt that Ampère had done considerable spadework prior to 1826. As Maxwell remarked in 1879:

We can scarcely believe that Ampère really discovered the law of action by means of the experiments which he describes. We are led to suspect, what, indeed, he tells us himself, that he discovered the law by some process which he has not shown us, and that when he had afterwards built up a perfect demonstration he removed all traces of the scaffolding by which he had raised it.⁴

Historians have recently taken up the task of reconstructing Ampère's discarded scaffolding. In particular, L. Pearce Williams and Christine Blondel have tried to unravel the chronology of the early events leading up to Ampère's discovery of electrodynamic forces between electric currents.⁵ Blondel has provided an accurate survey of Ampère's entire career in electrodynamics,⁶ and considerable attention has been given to Ampère's study of induction between 1820 and 1822.⁷

4 J. Maxwell, *A Treatise on Electricity and Magnetism*, 3rd edn., 2 vols, London, 1891; reprinted, New York, 1954, ii, pp. 175–176.

5 C. Blondel, 'Ampère and the programming of research', *Isis*, (1985), 76, pp. 559–561; L. Pearce Williams, 'What were Ampère's earliest discoveries in electrodynamics?', *Isis*, (1983), 74, pp. 492–508; L. Pearce Williams, 'Reply to "Ampère and the programming of research"', *Isis*, (1985), 76, p. 561.

6 C. Blondel, *A-M. Ampère et le Création de l'Électrodynamique*, Paris, 1982.

7 J. Hofmann, 'Ampère, electrodynamics, and experimental evidence', *Osiris*, (1987), 3 (in press); E. Mendoza, 'Ampère's experimental proof of his law of induction: $i_2\alpha i_1$ ', *European Journal of Physics*, (1985), 6, pp. 281–286; L. Pearce Williams, 'Why Ampère did not discover electromagnetic induction', *American Journal of Physics*, (1986), 54, pp. 306–311; L. Pearce Williams, 'Faraday and Ampère: a critical dialogue', in: D. Gooding and F. James (eds), *Faraday Rediscovered: Essays in the Life and Work of Michael Faraday 1791–1867*, New York, 1985, pp. 83–104.

The topic of the present essay is quite different in that I am concerned with Ampère's early experimental methods rather than any specific discovery as such. I intend to make explicit how some of Ampère's most creative experimentation in 1820 set an important precedent for his subsequent adoption of the equilibrium experiment technique. Prior to the middle of 1822 Ampère did not expect that this method could provide an adequate means for a full derivation of his force law; his first set of equilibrium experiments was not assembled until November 1825, and he continued to revise them thereafter. His 1826 memoir provides little information about how his initial responses to experimental novelty started this gradual implementation of a new experimental method. It is true that Ampère did begin his research with a preference for the primarily deductive mode of theory construction he eventually relied upon. Nevertheless, at the outset there was no reason for him to suspect that such a procedure would ever become applicable to the bewildering new domain of electrodynamics.

Ampère's experimental activities under these circumstances are intriguing in several respects. First, following his discovery of electrodynamic forces, Ampère's approach to experimentation was almost always guided by his predetermined tasks of specifying the mathematical expression for his electrodynamic force law and the structure of the electric circuitries he believed to exist within magnets. Thus, with rare but important exceptions, Ampère did not engage in experimentation with the exploratory mentality conducive to the pursuit of novelty for its own sake. Instead, his procedure was one in which appropriate new phenomena were, quite literally, to be *constructed* and then presented in an increasingly suggestive and clear manner so as to become readily accessible demonstrations fostering the acceptance of Ampère's theoretical interpretations. At the same time, as Ian Hacking has emphasized, 'experimentation has a life of its own';⁸ in his search for appropriate demonstrations, Ampère repeatedly encountered novel experimental events which, *malgré lui*, he had to recognize as unexpected discoveries. Secondly, the alacrity with which Ampère transformed seemingly anomalous observations into bases for further theoretical progress became the creative source for the type of demonstrations that he eventually developed into the equilibrium experiment technique.

This paper thus addresses the following questions concerning the first five months of Ampère's research on his electrodynamic force law. The time frame within which the questions are posed extends from September 1820 to February 1821; severe illness put a halt to Ampère's systematic activity for several months thereafter. First, what were Ampère's methodological views prior to 1820, and what bearing did they have on his early experimentation in electrodynamics? What effect did the methods and goals of other French physicists have on Ampère's conception of physical theory, and how did his rivalry with physicists such as Biot influence the course of his early research? Most importantly, how did Ampère transform initially anomalous experimental discoveries into the demonstration that later became the prototype for his equilibrium experiment technique? Finally, what indications are there that in 1821 Ampère did *not* yet foresee the full potential of the equilibrium method?

8 I. Hacking, *Representing and Intervening*, Cambridge, 1983, p. 150.

I. Ampère's methodological views prior to 1820

The most important source of information concerning Ampère's methodological views just prior to 1820 is a set of lecture notes compiled for part of a course in *logique* he taught at the Ecole Normale during 1817 and 1818.⁹ In particular, these notes reveal Ampère's sensitivity to the logical structure of inferences intended to link scientific theories to confirmatory experimental evidence. Ampère took great pains to distinguish four different argumentative modes; he soon would put two of them into practice in his own research in electrodynamics.

First, Ampère drew upon a long tradition to confront his students with yet another application of the time-honoured terms 'synthesis' and 'analysis'. He defined 'synthesis' as a mode of reasoning that proceeds from relatively 'simple' premises to conclusions that are more 'complicated'. 'Analysis', on the other hand, arrives at conclusions that are more simple than the original premises. The relative simplicity or complexity of a statement is dependent upon that of the state of affairs it describes. States of affairs are to be considered increasingly more complicated in-so-far as they are composite results of many individual causal agents acting under various attenuating circumstances.

For example, in the context of an imponderable fluid theory of electricity, a very simple statement would be the claim that the force between two particles of electric fluid is inversely proportional to the square of the distance between them. On the other hand, a claim that a large collection of electric fluid particles has reached a state of static equilibrium on the surface of a sphere obviously describes much more complicated circumstances resulting from the mutual interactions of many electric fluid particles.

Ampère next draws a second distinction between 'direct' and 'indirect' reasoning. Direct arguments proceed from premises that have been acknowledged to be true; they establish a conclusion that previously had been considered to be contingent. Indirect reasoning takes a contingent hypothesis as its major premise and derives a conclusion that can straightforwardly be recognized as true or false. Combining this dichotomy with that of analysis and synthesis results in four possible modes of reasoning: *direct analysis*, indirect analysis, direct synthesis and *indirect synthesis*.

For the physical sciences, the most important example of indirect reasoning is when a physicist, for example, tentatively adopts a simple hypothesis and derives complicated observable consequences from it that can be checked by experiment. In Ampère's terminology, this would be *indirect synthesis*. On the other hand, Newton's presentation of the universal law of gravitation is Ampère's favourite example of *direct analysis*. That is, one stage in Newton's argument involved taking as a premise the approximate truth of the relatively complicated statement of Kepler's third law in order to derive the more simple inverse square law for the attraction of each planet to the sun; he then could use the approximately spherical shape of the planets to infer the even more simple inverse square law for the gravitational attraction between any two arbitrarily small volumes of matter.

⁹ Ampère's notes for Lessons 25, 26 and 27 are most important and are to be found in the archives of the Académie des Sciences, carton 16, chemise 261.

Ampère made sure that his students realized that only after a preliminary stage of exploratory discovery has been completed can textbook writers use *direct synthesis* to give an axiomatic presentation of a theory for didactic purposes. The complex phenomena encountered in experimental research require either *direct analysis* or *indirect synthesis* as the only viable routes to the discovery and confirmation of simple theoretical laws. Ampère thus concentrated on these two modes and made some interesting remarks about his preferences. He pointed out that only in very rare cases might it be possible to emulate Newton's paradigmatic use of *direct analysis*; more commonly, recourse must be had to *indirect synthesis* and a theoretical claim must tentatively be adopted as a hypothesis. Two of Ampère's comments on this stratagem are worth quoting.

Cette nécessité de changer de methode n'est pas moins évident dès qu'il s'agit de . . . trouver l'explication d'un phénomène que la nature nous offre dans toute sa complication. Là les données étant par le fait même plus compliqués que les résultats qu'on cherche, la synthèse directe devient inapplicable, et il faut bien recourir soit à l'analyse directe si on le peut, soit à la synthèse indirecte du tâtonnement et des hypothèses explicatives. . . . Rappeler que pour remonter à la cause d'un ensemble de faits observés, il y a 2 marches—analyse directe, synthèse indirecte. Aveu que la première serait plus naturelle si l'on pouvait l'employer—efforts inutiles par quelques-uns mêmes des grandes hommes pour faire croire qu'ils l'avaient suivie.¹⁰

These passages provide a clear statement of an important methodological priority Ampère had adopted before beginning his research in electrodynamics. From Ampère's point of view, only *de facto* mathematical and physical complexity prevents scientists from following Newton's exemplary deductive use of *direct analysis*. The inductive confirmation of a hypothesis by repeated observation of its implications is less trustworthy; but this *indirect synthesis* is the typical pragmatic procedure in science.

Looking ahead to Ampère's own work in electrodynamics, it is clear that his eventual invention of the equilibrium experiment technique was fully in keeping with his preference for *direct analysis*. By deriving simple theoretical conclusions from complicated but clearly demonstrated experimental circumstances, Ampère could claim to have accomplished for electrodynamics the closest possible methodological correlation to Newton's theory of gravitation. Prior to his successful invention of this technique, Ampère was forced to rely upon *indirect synthesis* to confirm tentative hypotheses through observation of their consequences. My primary project in this essay is to show how Ampère's first recognition that equilibrium experiments might be a source for *direct analysis* emerged from his reaction to anomalous results produced during his practice of the hypothetico-deductive method of *indirect synthesis*. Ampère's enthusiasm for deductive formalism based upon a small set of well-chosen experimental demonstrations was not typical of the French scientific community in which his arguments would be scrutinized. But before turning to the 'Laplacian' scientific milieu in some detail, some additional points should be made about the goals and structure Ampère expected of a physical theory.

10 *Ibid.*

First, as Kenneth Caneva has made clear,¹¹ by 1816 Ampère had adopted Fresnel's luminiferous ether as an ontological basis for the unification of physics. From the time of his earliest recorded thoughts about physics, Ampère had been dissatisfied with the idea of forces acting instantaneously through space to an object located 'at a distance'.¹² One reason for Ampère's sudden and enthusiastic activity in electrodynamics in 1820 was that he hoped that it would offer new insights into the mechanics of an all-pervasive ether composed of superimposed negative and positive electric particles. His ultimate goal was that optics, electrostatics and electrodynamics (including magnetism) would all become domains determined by various types of etherial vibrations. Nevertheless, he recognized that this ambitious project would have to be preceded by a determination of the laws governing directly observable phenomena. Thus, although the ultimate goal of Ampère's research programme was a reduction to an ontology of electric fluids and the dynamics of their individual particles, he often used the term *fait primitif* or 'fundamental fact' to refer to the force he claimed to exist between any two 'infinitesimal' segments of an electric circuit. These segments actually contained the motions of many electric fluid particles, and the force between such segments thus was not truly 'primitive' from Ampère's point of view.¹³ Nevertheless, at this relatively crude ontological level, Ampère could still argue that his search for an electrodynamic force law was a natural extension of the French tradition in physics established and promoted by Coulomb, Hauy, Laplace, Biot and Poisson. This tradition deserves some detailed attention since the 'Laplacian' response to Oersted's discovery set the context for Ampère's own attempts to link theory to observation.

II. Laplacian physics and the Biot-Savart response to Oersted's discovery

A. Laplacian physics

In a series of thought-provoking essays during the 1970s, Robert Fox provided ample reasons for historians to recognize 'Laplacian physics' as a distinct and important phase in early nineteenth-century science.¹⁴ The full range of accepted conclusions and disputed questions generated by subsequent research in this area need not be surveyed here. I shall clarify the issues that are of particular importance for Ampère's electrodynamics and then focus on the Laplacian response to Oersted's 1820 discovery as carried out by Jean-Baptiste Biot and Félix Savart.

¹¹ Caneva, op. cit. (1).

¹² There are extensive manuscript notes on this subject preserved in the archives of the Académie des Sciences, carton 10, chemise 203. They can be dated as written between 1801–1802.

¹³ Ampère emphasized this point in several publications including his famous 1826 memoir, op. cit. (2). The most commonly cited version of this monograph is the slightly revised 1827 publication: 'Mémoire sur la théorie mathématique des phénomènes électro-dynamiques, uniquement déduite de l'expérience . . .', *Mémoires de l'Académie Royale des Sciences de l'Institut de France. Année 1823, (1827)*, 6, pp. 175–387, on p. 294.

¹⁴ R. Fox, 'The rise and fall of Laplacian physics', *Historical Studies in the Physical Sciences*, (1974), 4, pp. 89–136; R. Fox, 'Scientific enterprise and the patronage of research in France 1800–70', *Minerva*, (1973), 11, pp. 442–473.

Although Laplacian physics was practised most actively between 1800 and 1820, it owed much of its initial inspiration to Charles Coulomb's careful experimental determination of the inverse square distance dependency laws for electric and magnetic forces between 1785 and 1789. Thereafter, Haüy, Laplace, Biot and Poisson became spokesmen for what Biot called a *renaissance de la véritable physique*.¹⁵ Following Coulomb's example, this *renaissance* was promoted as a return to a rigorous physics epitomized by Newton's theory of gravitation; its hallmark was to be the accurate confirmation of mathematically formulated laws by quantitative experimental measurements. In 1817, Biot gave the following summary of his conception of proper scientific procedure in one of his influential textbooks:

L'étude de la nature physique, considérée dans toute son étendue, se réduit toujours à ces trois choses distinctes: l'observation des phénomènes; la recherche expérimentale du mode suivant lequel ils s'accomplissent, et qui est leur loi physique; enfin, la détermination des forces abstraites et mécaniques dont ils résultent comme conséquences calculables.¹⁶

Laplacian physicists thus did not restrict themselves to the discovery of empirical relationships governing directly observable phenomena; attractive and repulsive forces were their most fundamental explanatory principles.

However, as the history of physics so amply demonstrates, explanations based upon forces inevitably evoke further questions concerning the ontology from which these alleged forces arise. In this respect, Laplacian physics underwent a major transition in 1812. At that point Poisson published two important memoirs on electrostatics in which he calculated an impressively detailed set of consequences based upon the hypothesis of inverse square-distance dependent forces acting between particles of two imponderable electric fluids.¹⁷ The close match between these theoretical calculations and the extensive data inherited from Coulomb made the existence of imponderable fluids an integral part of Laplacian physics thereafter. Earlier scepticism about the reality of unobservable causes now gave way to a systematic attempt to repeat Poisson's success in electrostatics within the domains of optics, heat and magnetism.¹⁸

Biot, for example, had only been willing to accept the existence of imponderable particles in optics prior to 1812; thereafter he became one of the most assertive proponents of the new point of view. In a passage written in 1816, he made an important point about the distinct domains determined by the apparent lack of interaction between the various imponderable fluids. For Biot, physics should be limited to the study of:

les phénomènes produits par les actions des principes invisibles, intangibles, impondérables, tels que l'électricité, le magnétisme, le calorique et la lumière. Comme jusqu'à présent ces principes

15 J.B. Biot, 'Borda', *Biographie Universelle*, 5, (ed. Michaud), Geneva, 1812; 1854 edition, p. 60. For a good discussion of the context for Biot's phrase, see E. Frankel, 'J.B. Biot and the mathematization of experimental physics in Napoleonic France', *Historical Studies in the Physical Sciences*, (1977), 8, pp. 33–72.

16 J.B. Biot, *Précis Élémentaire de Physique Expérimentale*, 2 vols, Paris, 1817; 2nd edn., 1821; 3rd edn., 1824; quoted passage from 3rd edn., vol. 1, p. 5.

17 For a good discussion of the impact of Poisson's memoirs, see R.W. Home, 'Poisson's memoirs on electricity: academic politics and a new style in physics', *British Journal for the History of Science*, (1983), 16, pp. 239–259.

18 For a thorough analysis of Poisson's contributions to the physics of ponderable matter, see D.H. Arnold, 'The Mécanique Physique of Simeon Denis Poisson: the evolution and isolation in France of his approach to physical theory (1800–1840)', *Archive for History of Exact Sciences*, (1983), 28, pp. 243–367; (1983), 29, pp. 37–51; (1984), 29, pp. 287–307.

se distinguent par leurs effets, ou par les conditions sous lesquelles ils agissent, sans qu'on sache encore s'il est possible de les réduire les uns dans les autres, leur étude doit nécessairement former quatre divisions distinctes, en observant toutefois de faire ressortir avec soin toutes les analogies qui pourrrent servir un jour de les rapprocher.¹⁹

As became evident after 1820, this division of physics into disjoint domains was more important to Biot than the above passage might indicate; his reaction to Oersted's discovery would be guided by the presupposition that the electric and magnetic fluids do not interact.²⁰

However, before turning to Biot's confrontation with electrodynamics, an important methodological issue should be noted. We have seen how Ampère demoted the hypothetico-deductive method of *indirect synthesis* to the status of a pragmatic expedient to be adopted when the complexity of the observed phenomena makes a *direct analysis* impossible. On the other hand, Biot drew a distinction between the gradual inductive discovery of empirical laws relating directly observable phenomena and the hypothetico-deductive confirmation of more elementary forces pertaining to unobservable causes. During the height of the 1821 debates in optics and electrodynamics, Biot summarized his views as follows:

L'observation, quelquefois le hasard, découvrent les phénomènes; la méthode expérimentale les développe et détermine leurs lois physiques: mais le dernier mystère des forces élémentaires qui les produisent, ne peut être mis en évidence que par la puissance de la pensée.²¹

Biot thus expected the justification of elementary force laws pertaining to magnetic and electric fluids to come about through a successful match between measurements and calculations based upon speculative hypotheses. His exemplar was Poisson's electrostatics, and he had no expectations that anything resembling Ampère's *direct analysis* was an option for a practising physicist.

On the other hand, Biot's emphasis on 'elementary forces' paralleled Ampère's demand for the discovery of a *fait primitif* as a causal basis for more complicated phenomena within any experimental domain. Although Ampère ultimately hoped to locate the most elementary causes of electric and electrodynamic phenomena in oscillatory ethereal motions, this project had to be preceded by the determination of the laws governing the directly observable effects produced by electric circuits. Within this relatively crude ontological context, Ampère argued that the 'fundamental fact' of electrodynamics was a central force acting along the line joining any pair of small current elements; Biot opted for a transverse force produced by a current element acting on a magnetic fluid particle. Biot's attempts to legitimate this claim should be discussed in some detail since they created a hostile conceptual and methodological context for Ampère's own research.

19 J.B. Biot, 'Lettre de Mr. Biot, membre de l'Académie des Sciences, etc. au Prof. Pictet, correspondant de l'Académie et l'un des rédacteurs de ce recueil', *Bibliothèque Universelle des Sciences, Belles-Lettres, et Arts*, (1816), 2, pp. 81–86, on pp. 84–85.

20 For example, see the following announcement written in November 1820: J.B. Biot, 'Avertissement sur la seconde édition du *Traité de Physique Élémentaire* de M. Biot', *Annales de Chimie et de Physique*, (1820), 15, pp. 331–335.

21 J.B. Biot, 'Sur l'aimantation imprimée aux métaux par l'électricité en mouvement: lu à la séance publique de l'Académie des Sciences, le 2 avril, par M. Biot', *Journal des Savans*, (1821), Avril, pp. 221–235, on p. 235.

B. The Biot-Savart response to Oersted's discovery

Although most French physicists initially were sceptical about Oersted's claim that an electric current could change the orientation of a suspended magnet, Arago established the legitimacy of the new effect at a public demonstration for the Académie des Sciences on 4 September 1820. Oersted's report had not been clear about the magnitude of the reorientation; during the next two weeks Ampère discovered that when the interference of terrestrial magnetism is eliminated, the magnet takes an orientation normal to a line drawn from its centre to the conducting wire.²²

Biot's response to these events was quite in keeping with his penchant for quantitative data as a necessary basis for the determination of relevant force laws. As he wrote in retrospect in 1821:

La première chose qu'il falloit découvrir, c'étoit la loi suivant laquelle la force émanée du fil conjonctif s'affoiblit à diverses distances de son axe.²³

To determine how the magnitude of the Oersted effect varied with changes in the initial conditions, Biot and Félix Savart²⁴ magnetized a small steel needle and suspended it in a horizontal plane. An appropriately located bar magnet cancelled out the magnetic effect of the earth, and the magnetized needle was then exposed to a long vertical conducting wire located at a carefully measured distance. It was observed that the needle always came to equilibrium with an orientation perpendicular both to the wire and to the line joining the needle to the wire. To study this effect quantitatively, Biot noted that just as a pendulum can be used to measure the strength of the earth's gravitational force, either the frequencies or the periods of the oscillations of the needle, when displaced from its equilibrium position, can be used to compare the strengths of the force bringing the needle back to its position of equilibrium for various separations of the wire from the needle. The operative force is proportional to the square of the frequency or inversely proportional to the square of the period.

In their first short publication, Biot and Savart reported their results as follows:

A l'aide de ces procédés, MM. Biot et Savart ont été conduits au résultat suivant qui exprime rigoureusement l'action éprouvée par une molécule de magnétisme austral ou boréal placée à une distance quelconque d'un fil cylindrique très-fin et indéfini, rendu magnétique par le courant voltaïque. Par le point où réside cette molécule, menez une perpendiculaire à l'axe du fil: la force qui sollicite la molécule est perpendiculaire à cette ligne et à l'axe de fil. Son intensité est réciproque à la simple distance.²⁵

Biot and Savart obviously carried out quite a complicated set of inferences in order to be able to offer the above statements as the 'result' of their observations. Most importantly, they claimed that they had discovered that the wire exerts a force on

22 L.P. Williams has recently clarified this point; see Williams, op. cit. (5), 1983, p. 498.

23 Biot, op. cit. (21), p. 228.

24 Félix Savart published extensively in the areas of acoustics and elasticity. Biot helped him to become established within the Parisian scientific community, and Savart's interest in electromagnetism seems to have been limited to this early stage in his career. See S. Dostrovsky, 'Félix Savart', *D.S.B.*, vol. 12, pp. 129–130.

25 J.B. Biot and F. Savart, 'Note sur le magnétisme de la pile de Volta', *Annales de Chimie et de Physique*, (1820), 15, pp. 222–223, on p. 223; also published as 'Sur la mesure de l'action exercée à distance sur une particule de magnétisme', *Journal de Physique, de Chimie, d'Histoire Naturelle et des Arts*, (1820), 91, p. 151.

individual particles of magnetic fluid; their actual measurements, of course, pertained only to the directly observable effect produced on the entire magnetized needle. As resolute champions of the two-fluid theory of magnetism, Biot and Savart assumed that, although the two types of magnetic fluid particles were constrained within individual molecular regions, the net effect of their polarized separation within those regions would be the same as that which would be produced by a concentration of one fluid near each pole of the magnet. By approximating the distance from the wire to each of these poles by its distance from the centre of the magnet, the experimentally determined action of the wire on the magnet could be interpreted in terms of action on magnetic fluid particles located at each pole. The observed inverse proportionality to distance was thus said to apply to a force acting on individual magnetic fluid particles.

Following their presentations of these conclusions to the Académie on 30 October, Biot and Savart carried out a second set of measurements using wires still positioned in a vertical plane but with a single bend at the height of the suspended magnet. Based upon the slightly retrospective account Biot presented in April 1821, the new conclusions he presented to the Académie on 18 December can be summarized as follows.²⁶ First, the initial set of data with straight wires now was specifically interpreted as measurements of a *résultat composé* brought about by the action of the entire wire on a particle of magnetic fluid. Second, the inverse dependency of this total force with respect to distance from the wire could be shown to be a mathematical consequence of an inverse square distance dependency for more elementary forces assumed to act upon the magnetic fluid particle from each ‘infinitesimal’ segment of the wire. The second set of measurements with bent wires then was interpreted as evidence that these elementary segmental forces were also proportional to the sine of the angle formed at the point where a line drawn from the magnetic particle to the wire segment meets the wire.

Biot’s justification for these claims was an interesting combination of rhetoric and error. First, he claimed that Laplace had ‘deduced’ the inverse square dependency of the segmental force from Biot’s first set of data.²⁷ More accurately, Laplace’s claim was simply a hypothesis confirmed by Biot’s data; in Ampère’s terms this was *indirect synthesis* rather than *direct analysis*. Secondly, the angular dependency of the segmental force did not really account for Biot’s original report of his second set of data; he corrected this several years later by altering the empirical law he had used to summarize his measurements.²⁸ In any event, in spite of his claim that his conclusion about the angular dependency of his segmental force had been ‘analysed by calculation’,²⁹ Biot again had been practising Ampère’s *indirect synthesis*; he assumed that the segmental

26 To my knowledge, no direct report of Biot’s 18 December presentation has survived. Biot gave a fairly detailed summary of both sets of measurements at the public session of the Académie on 2 April 1821, and he published this discourse shortly thereafter; see Biot, op. cit. (21). Most of this publication was incorporated into the second edition of Biot’s *Précis*, op. cit. (16), and was expanded into a much larger version in the 1824 third edition.

27 Biot, op. cit. (21), p. 229.

28 Biot, op. cit. (16) 3rd edn., vol. 2, pp. 742–746; the relevant passages from the 1821 and 1824 editions are conveniently contrasted in *Mémoires sur l’Électrodynamique*, vols 2 and 3 of *Collection de Mémoires Relatifs à la Physique*, 5 vols, Paris, 1885–1887; vol. 2, pp. 116–120.

29 Biot, op. cit. (16), 2nd edn., vol. 2, p. 123.

force was proportional to the sine of the relevant angle and then derived a measurable implication from this hypothesis. The second set of data he and Savart had gathered was used as confirmatory evidence. This data actually only directly pertained to the total composite action of conducting wires on magnets. Biot's references to 'deduction' and 'analysis' of more elementary forces were simply rhetorical attempts to lend an aura of certainty to what were actually inductive confirmations of hypotheses. The systematic quantitative procedure of this confirmation was precisely the *indirect synthesis* Ampère expected to be necessary in the new domain of electrodynamics.

Before turning to Ampère's activities in detail, Biot's ultimate ontological commitments should be reiterated. Although he claimed to have discovered a mathematical expression for forces assumed to act upon magnetic fluid particles from each tiny section of a conducting wire, Biot considered this to be only a preliminary step towards a full explanation based upon an understanding of 'une véritable aimantation moléculaire imprimée aux particules des corps métalliques par le courant voltaïque qui les traversait.'³⁰ The most elementary forces for Biot thus were assumed to act between molecular fluid particles in the wire and in the magnet. Not surprisingly, Biot never explained how a wire's 'molecular magnetism' was produced without violating the conventional Laplacian prohibition of any interaction between electric fluid and magnetic fluid. Instead, he concentrated upon arguing that his segmental force could at least account for electrodynamic phenomena on a less fundamental level. Ampère's fledgling attempts to create his own physics of electrodynamics thus took place in competition with a powerful rival research programme headed by one of France's most prestigious scientists.³¹

Ultimately, Biot and Ampère were determined to reduce electrodynamics to the action of magnetic or electric fluid, respectively. Meanwhile, given that this issue could not be resolved experimentally in 1820, the rivalry demanded a choice for one of two alternative 'fundamental facts' as a temporary but important basis for explanation at an ontological level that actually was not considered to be fundamental or final.

With this context in mind, we should expect that as Ampère searched for evidence for his electrodynamic force law, he was also constantly alert for any opportunity to convert initially perplexing experimental events into demonstrations readily interpreted in terms of his own research programme. This was indeed the scenario in which he took his first step towards a *direct analysis* of his force law through the construction and interpretation of equilibrium phenomena.

III. Ampère's initial production of equilibrium phenomena

Now that the Laplacian conceptual and methodological context has been clarified, Ampère's efforts to discover and justify a mathematical expression for his electrodynamic force law can be studied in detail. My primary goal is to explain how Ampère's

30 Biot, op. cit. (16), 3rd edn., vol. 2, p. 773, and as quoted in *Mémoires*, op. cit. (28), vol. 2, p. 127.

31 The rivalry had become quite heated by the time of the annual public session of the Académie on 2 April 1821. Biot took the occasion to claim that Ampère's discovery of attractions and repulsions between conductors should be understood in terms of the magnetic states of the conductors. See Biot, op. cit. (21).

encounter with experimental anomaly during 1820 was the instigation for his invention of the apparatus that became an early prototype for the equilibrium experiment technique he explicitly promoted during 1822. I begin with Ampère's formulation of a tentative hypothesis during October 1820. I describe his original plans to test the implications of this hypothesis and his attempts to duplicate the action of magnets using electric circuits. These efforts led to unexpected experimental events which Ampère soon construed in terms of what I refer to as his 'addition law' for electric circuit elements. On 4 December 1820, Ampère used this law as a premise in his first public derivation of the angular factor in his force law. During the following month, he invented an apparatus designed to produce a suggestive state of equilibrium, and he argued that this phenomenon should be interpreted as an experimental basis for the addition law. This was the beginning of Ampère's gradual replacement of *indirect synthesis* by *direct analysis*, a transition he did not advance further until over a year later. A brief chronology of particularly relevant events is provided in an appendix.

A. Ampère's first force law hypothesis: October 1820

As Kenneth Caneva has argued in great detail,³² Ampère's enthusiastic reaction to Oersted's discovery was facilitated by his lack of commitment to Laplacian concepts and institutions. Rather than try to incorporate the new effect into one of the disjoint Laplacian categories of magnetism or *static* electricity, Ampère interpreted it as an indication that a new type of force arises due to *moving* electricity. For Ampère, magnetic fluid simply does not exist; instead, magnetic effects are produced by closed electric circuits within planes normal to the axes linking the poles of magnetized bodies.

This implied that it might be possible to duplicate the interactions between magnetic poles using conducting wires coiled into spirals; it was probably during such an investigation that Ampère discovered the existence of attractive and repulsive forces between linear wires bearing electric currents.³³ By the 9 October meeting of the Académie des Sciences, he used the apparatus shown in Fig. 1 to demonstrate an attraction between parallel linear conductors bearing currents in the same direction and a repulsion when currents flow in opposite directions. AB is a fixed conductor, and CD is free to swing on pivots located at E and F.

In keeping with the reductionist mentality he shared with his Laplacian colleagues, Ampère saw his next task to be the formulation of a mathematical law for an electrodynamic force acting between any pair of electric circuit segments small enough to be considered 'infinitesimal' in comparison to measurable magnitudes. In principle, such a formula could be mathematically integrated over the path of any electric circuit,

32 Caneva, op. cit. (1) and Caneva, 'What should we do with the monster? Electromagnetism and the psychosociology of knowledge', in: E. Mendelsohn and Y. Elkana (eds) *Science and Cultures, Sociology of the Sciences*, Dordrecht, 1981, 5, pp. 101–131.

33 See Williams, op. cit. (5), 1983 for evidence on this point.

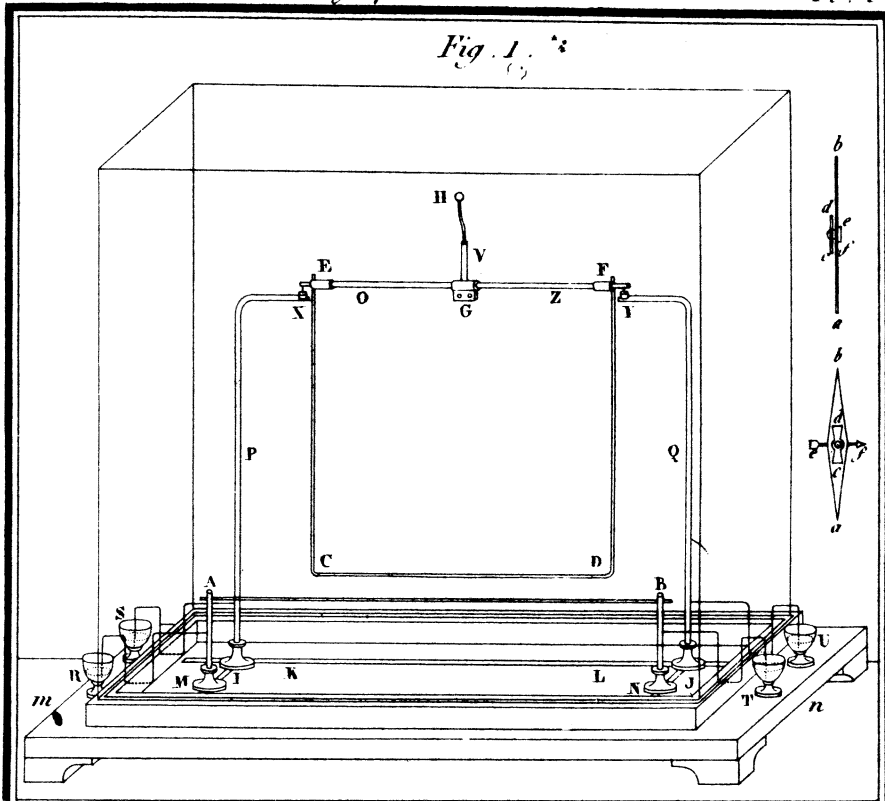


Figure 1. Ampère's apparatus for demonstration of forces between parallel linear currents.

including those Ampère believed to exist within magnets.³⁴ Precedents set in other physical domains by Poisson, Biot, Laplace, Fourier and Fresnel encouraged Ampère's unhesitating resolution of an electric current into a collection of elementary components.

The most important source of information concerning Ampère's earliest research on his force law during October 1820 is one of the many fragmentary manuscript drafts composed during this period.³⁵ From this document we learn that Ampère assumed that

34 As early as the 8 January 1821 meeting of the Académie, Ampère proposed that the electric currents within magnets might be confined to circuits around individual molecules. This was only presented as a possibility at this point due to lack of experimental evidence and because Ampère assumed that the summation of these molecular currents would be equivalent to large scale currents to which he could more readily apply his force law. The notes for Ampère's lecture are preserved in the archives of the Académie des Sciences, carton 8, chemise 166.

35 This important manuscript fragment seems to be a rejected draft for the concluding part of the long memoir Ampère eventually presented to the Académie on 26 December 1820: Ampère, 'Suite du mémoire sur l'action mutuelle entre deux courans électriques, entre un courant électrique et un aimant ou le globe terrestre, et entre deux aimans', *Annales de Chimie et de Physique*, (1820), 15, pp. 170–218. The manuscript is preserved in the archives of the Académie des Sciences, carton 8, chemise 158. It has been partially edited in C. Blondel, 'Sur les premières recherches de formule électrodynamique par Ampère (octobre, 1820)', *Revue d'Histoire des Sciences et Leurs Applications*, (1978), 31, pp. 53–65.

the force should be an attractive or repulsive force directed along the line linking two circuit elements.³⁶ Secondly, he speculated that the force should vary as the inverse square of the separation of the elements, 'conformément à ce qu'on observe pour tous les genres d'actions plus ou moins analogues à celui-là'.³⁷

For the special case where both elements are perpendicular to the line connecting them, Ampère drew some implications from his observations of the relative magnitudes of the forces between *finite* circuit components with this orientation. He had noticed that the force was maximally attractive for parallel currents, became zero when they were mutually perpendicular, and became increasingly repulsive when they are anti-parallel. Ampère argued that the total force between finite circuit components should thus include a factor which was a function of odd degree in the cosine of the angle between the directions of the two currents. The manuscript account then continues as follows:

Au reste, cette fonction du cosinus de l'angle de la direction des deux courants électriques ne peut avoir une forme simple que quand on considère des portions infiniment petites de ces courants. Il est probable qu'elle se réduit alors à la première puissance de ce cosinus, et c'est du moins la première supposition qu'il convient d'essayer dans la comparaison d'une hypothèse sur la loi des attractions et répulsions avec les résultats de l'expérience.³⁸

Ampère then speculated further that, in the more general case where the two elements are not perpendicular to the line joining them, the force should depend upon the angles α and β which they make with this line. Also, the cosine of the angle γ between two planes, each of which passes through the connecting line and one of the elements, should replace the cosine of the angle between the direction of the two currents which was argued for in the simpler case. The three angles α , β and γ are exemplified in Fig. 2.

Although Ampère did not leave any record of the full mathematical expression he constructed at this point, his original thoughts concerning the dependency of the force on the angles α and β were probably those registered by Babinet in the *Exposé des Nouvelles Découvertes sur le Magnétisme et l'Électricité*. Although not published until early in 1822, the *Exposé* was almost completed by July 1821.³⁹ In sections 15 and 16 Babinet included an analysis of how the electrodynamic force should vary as a function

36 When Ampère began teaching physics at the Collège de France in November 1824, he and Joseph Liouville composed a long manuscript draft entitled 'Théorie mathématique des phénomènes électrodynamiques'. Although never published, this treatise includes a symmetry proof by Liouville in support of Ampère's longstanding assumption that the electrodynamic force lies along the line joining two current elements. The manuscript is in the archives of the Académie des Sciences, carton 11, chemise 208 bis.

37 Archives of the Académie des Sciences, carton 8, chemise 158; edited by Blondel, op. cit., (35) p. 64.

38 Ibid. A similar argument appears in one of Ampère's earliest publications in electrodynamics: 'Analyse des mémoires lus par M. Ampère à l'Académie des Sciences dans les séances des 18 et 25 septembre, des 9 et 30 octobre 1820', *Annales Générales des Sciences Physiques*, (1820), 6, pp. 238–257, on pp. 247–248.

39 A-M. Ampère and J. Babinet, *Exposé des Nouvelles Découvertes sur l'Électricité et le Magnétisme de MM. Oersted, Arago, Ampère, H. Davy, Biot, Erman, Schweiger, De La Rive, etc.*, Paris, 1822. Ampère mentions the completion date in a 23 January 1822 letter to Faraday edited in S. Ross, 'The search for electromagnetic induction 1820–1831', *Notes and Records of the Royal Society*, (1965), 20, pp. 184–219; on pp. 217–218. Babinet composed most of the *Exposé* but with important additions and corrections by Ampère. A valuable set of proof sheets for the first nineteen sections has survived; these provide considerable insight into Ampère's thinking during this period, particularly in the case of passages which Ampère did not correct at this time but which he later realized were incorrect; archives of the Académie des Sciences, carton 11, chemise 208.

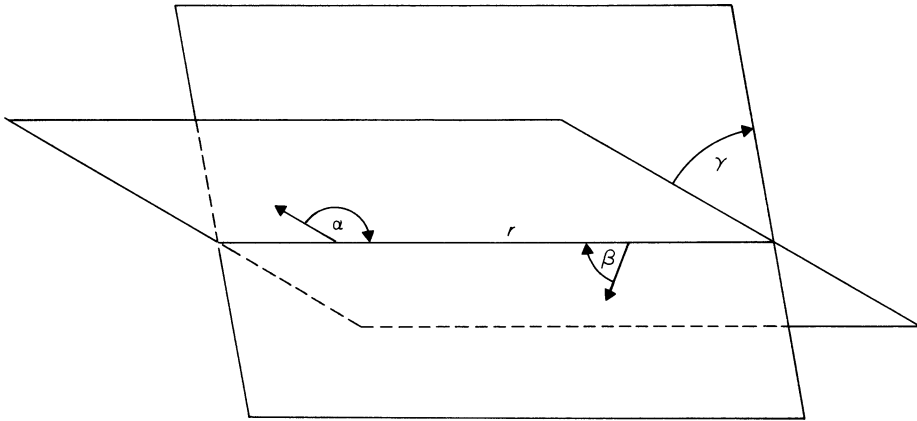


Figure 2. Ampère's representation of the mutual orientation of two current elements: α , β and γ .

of the angle α , and his argument parallels the reasoning Ampère had applied to observations of how the forces between finite conductors vary with the angle γ . Babinet concluded that the force between two current elements vanishes whenever one of them is directed along the line connecting them.⁴⁰ Ampère apparently had again drawn a conclusion from his investigation of forces between circuit components large enough to be individually manipulated. Combining this reasoning with the manuscript discussion of the dependency on $\cos \gamma$, it is very likely that as early as mid-October 1820 Ampère had concluded that the force between current elements was proportional to the following expression:

$$r^{-2} \sin \alpha \sin \beta \cos \gamma. \quad (1)$$

This would make the electrodynamic force vanish whenever α or β becomes zero, that is, when at least one of the current elements is directed along the line connecting them.

It is clear that Ampère had arrived at this tentative hypothesis by relying upon what he considered to be reasonable presuppositions and plausible conjectures suggested by a few simple observations. The contrast between his procedure and that of Biot is striking. Nevertheless, at this point Ampère still expected that he too would necessarily have to amass a large collection of quantitative data as confirmatory evidence; *indirect synthesis* seemed to be the only applicable method of justification.

B. Ampère's initial plans to provide evidence for his hypothetical force law; October 1820

With a plausible force hypothesis in hand, Ampère dedicated much of the second half of October to designing and constructing experimental apparatus. Unlike Faraday, Ampère

⁴⁰ Ampère and Babinet, op. cit. (39), p. 19.

left no systematic daily record of his research. A reconstruction of the chronology of his activities must be based upon a judicious assessment of sketchy manuscript notes and subsequent recollections that are not always entirely trustworthy. We do know that as early as 25 September he had already started experimental investigations of the second major tenet of his electrodynamics, his conviction that magnetic phenomena are caused by electric circuits in planes normal to the axis linking magnetic poles. Expecting to be able to duplicate the action of magnets using appropriately designed electric circuits, Ampère began by coiling conducting wires into spirals and helices. The planar spirals were intended to imitate circuitry at the pole of a bar magnet, and helices represented the circuitry of the entire magnet.

Ampère's earliest helices were balanced on a pivot like that of a compass needle; perhaps as early as 25 September he found that this apparatus responded as expected to the presence of a bar magnet.⁴¹ This was encouraging, but Ampère could not detect any corresponding response to the directive action of terrestrial magnetism. Suspecting that friction was preventing his helix from rotating, Ampère switched to a more mobile suspension by late September.⁴²

As shown in Fig. 3, the helix now is centrally suspended by a vertical extension of the same wire that makes up the helix. This is accomplished using a circuit in which the helix

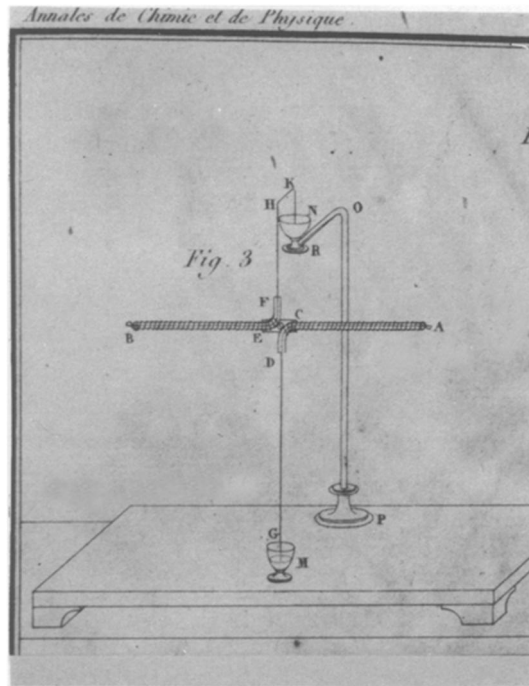


Figure 3. One of Ampère's axially compensated helices.

41 Ampère, op. cit. (38), p. 240, and 'Suite de mémoire sur l'action mutuelle entre deux courans électriques, entre un courant électrique et un aimant ou le globe terrestre, et entre deux aimans', *Annales de Chimie et de Physique*, (1820), 15, pp. 170–218; on p. 172.

42 Ampère, op. cit. (41), 'Suite', p. 173.

is assembled using two glass tubes. After descending vertically to the central point of suspension, the wire goes inside one of the tubes and passes along its axis to the end of the tube. There it emerges and is wrapped around this tube so as to return to the central point. The wire is then wrapped around the second tube, enters the end of the tube, passes back along the axis of the tube to the central point again, and then descends vertically. The circuit thus includes a two-part helix enclosing an axial current flowing in the direction opposite to the longitudinal components of the helix spires. Ampère assumed that neither the axial current nor the longitudinal components of his helix spires would interfere with the earth's directive action on the circular currents of the helix which were intended to duplicate the circuitry of a bar magnet.

Nevertheless, in spite of its greater mobility, this device also failed to respond to terrestrial magnetism; Ampère thus was confronted by a serious anomaly for his theory of magnetism.

This situation persisted during the first two weeks of October, the period in which Ampère formulated his initial force law hypothesis. By the middle of the month he had designed an apparatus to start testing the accuracy of his formula, and he described his plans in a manuscript draft that he probably composed late in October. Some of these passages are worth quoting in length.

C'est sur ces considérations generales que j'avais construit une expression de l'attraction de deux courans infiniment petits qui n'était à la vérité qu'une hypothèse, mais la plus simple qu'on put adopter, et celle par conséquent qu'on devait d'abord essayer. J'ai cherché à en tirer les effets qui devait en résulter, tant pour des courans électriques rectilignes mais d'une grandeur finie, que pour des courans circulaires comme ceux que j'ai montrés exister dans les aimants cylindriques, et pour les courans qui ont lieu dans des fils de cuivre pliés en hélice, à cause des expériences variées que j'avais faites sur cette dernière sorte de courans. Je me proposais de comparer les résultats de ces calculs avec des expériences où l'on put mesurer l'intensité d'action de deux courans rectilignes, (d')une longueur finie et dont l'angle pouvait varier à volonté; . . . J'avais construire pour ces mesures un appareil que j'ai montré le 17 octobre dernier à MM. Biot et Gay-Lussac, je m'en suis procuré un autre pour observer l'action de deux courans pliés en hélice.

Les expériences que j'essayai avec ces deux instruments me firent découvrir deux faits nouveaux qui en compliquaient les résultats et m'obligèrent par conséquent à suspendre les vérifications qu je m'étais proposé de faire, à l'aide de ces appareils, des résultats de mes calculs.⁴³

Both of Ampère's *deux faits nouveaux* were of major importance for his subsequent research; in each case a discovery resulted from the unexpected behaviour of new apparatus. One of these discoveries became the instigation for Ampère's interest in equilibrium demonstrations. His other discovery should be discussed first, however, because this will make clear how Ampère originally intended to accumulate evidence for his force law hypothesis in October.

The apparatus involved was the one Ampère claims to have shown to Biot and Gay-Lussac on 17 October. It was similar to that shown in Fig. 1 except that the originally fixed conductor AB was replaced by one that could be given a variable

43 Ampère, Archives of the Académie des Sciences, carton 8, chemise 158; edited by Blondel, op. cit. (35), p. 65. A revised version of part of this passage appeared in Ampère's 1820 memoir, op. cit. (41), p. 182.

orientation in a vertical plane.⁴⁴ By varying the orientation of this conductor, Ampère intended to compare observed effects on the mobile conductor CD with calculations based upon the application of his force hypothesis. However, during his preliminary investigations he was surprised to see the mobile conductor influenced by terrestrial magnetism; he quickly realized that his helix devices had failed to respond in this way due to their small radial dimensions.⁴⁵ A serious anomaly thus was resolved by a discovery using apparatus designed for an entirely different purpose. Subsequently, in order to compensate for the magnetic action of the earth on large suspended current loops, Ampère simply used pairs of loops traversed by currents in the proper directions so that the torques produced on the two loops by terrestrial magnetism are in opposite directions. Using this method he designed a replacement for the first uncompensated apparatus he had shown Biot and Gay-Lussac on 17 October; thus, he was in a position to settle down to a laborious comparison of calculations and data which would hopefully provide confirmatory evidence for his force hypothesis. There is no indication that Ampère ever put this project of *indirect synthesis* into practice. Instead, his research took quite a different direction due to the second of the *faits nouveaux* he probably discovered shortly after 17 October.

C. Anomalous helices and Ampère's addition law: October–November 1820

To review, on 9 October Ampère had publicly displayed repulsions and attractions between linear conductors using the apparatus shown in Fig. 1. Secondly, perhaps as early as 25 September he had been using centrally suspended helices coiled around axial currents, as shown in Fig. 3, to produce the expected rotational reorientation of a helix due to the attraction or repulsion of one of its ends by the pole of a bar magnet.

At some point during the hectic month after 25 September, Ampère also attempted to demonstrate how the mutual action of two helices duplicates that of two bar magnets. He left no record of when he did this for the first time; in his most detailed discussion of the subject he reported that he made the test by using helices in place of the linear conductors in the apparatus shown in Fig. 1.⁴⁶ Although this instrument was not shown to the Académie until 9 October, it is possible that Ampère built it and modified it with helices at some point during the preceding two weeks. An early date is suggested by Ampère's report that he had made the resulting observation 'long-temps avant d'en connoître la cause'.⁴⁷ On the other hand, in what is probably the earliest relevant manuscript report,

44 Ampère, op. cit. (41), p. 182.

45 Ibid.

46 Ampère, op. cit. (41), p. 173.

47 Ampère, 'Notes de M. Ampère sur les lectures qu'il à faites à l'Académie des Sciences', *Journal de Physique, de Chimie, d'Histoire Naturelle et des Arts* (abbreviated hereafter as *Journal de Physique*), (1820), 91, pp. 166–169; on p. 168. Ampère's claim about this observation and much of the rest of the chronology in 'Notes' were repeated in G. de Laumont, 'Note sur les expériences électro-magnétiques de MM. Oersted, Ampère et Arago, relatives à l'identité de l'aimant avec l'électricité', *Annales des Mines*, (1820), 5, pp. 535–546; on p. 544. Ampère identifies Laumont as the author of this unsigned report in a 21 February 1821 letter to Roux-Bordier; *Correspondance du Grand Ampère*, L. de Launay (ed.), 3 vols, Paris, 1936–1943 (abbreviated hereafter as *Correspondance*), vol. 2, p. 567.

Ampère implies that this observation gave him the second of the *faits nouveaux* that delayed his plans to verify calculations based upon his hypothetical force law.⁴⁸ Since Ampère explicitly says that he showed the apparatus responsible for one of these 'new facts' to Biot and Gay-Lussac on 17 October, this seems to imply that neither of the two discoveries took place until after that date. In short, the available evidence does not allow any clear-cut determination of the date of Ampère's first experiment with two helices; the reference to 17 October and the apparatus involved make the second half of October a plausible conjecture.⁴⁹

Ampère obviously was not concerned about leaving a precise chronology of his early research. In the present case this is particularly unfortunate; Ampère's second 'new fact' constituted a serious anomaly for his conviction that magnetic phenomena could be duplicated using spirals and helices. Much to his surprise, when he replaced the linear conductors in the apparatus of Fig. 1 by simple helices wound around glass tubes without axial currents, he found that the two parallel helices duplicated the interactions of two linear currents rather than two bar magnets. That is, they attracted each other when Ampère expected them to repel and vice versa. Ampère's reaction to this discovery was typical of his general attitude towards experimental novelty. Considered simply as a 'new fact', the unforeseen event would have held little in interest for him if it had not violated his theoretical expectations. Faced with an apparently serious anomaly, Ampère did not publicize it until he could produce other phenomena to bolster an argument for how the anomaly could be defused and transformed into confirmatory evidence.

This process began when Ampère noticed how the misbehaving helices differed from those he had been using hitherto. Up to this point, Ampère had assumed that, when he coiled a wire into a helix to imitate a magnetic circuitry, the longitudinal dimension of each spire was too small to have any noticeable effect. Probably during the last week in October or the first week in November, Ampère decided that this assumption had been incorrect; he now argued that the unexpected behaviour of his helices was due to the cumulative effect of the longitudinal components of the spires being equivalent to a linear current along the axis of each helix. In apparatus such as that shown in Fig. 3, Ampère had unwittingly compensated for this longitudinal current by running a current back down the axis of the helix in a direction opposite to that followed by the longitudinal components of the spires. He had done this simply to implement his new suspension method or, as he put it, '*sans que j'en prévu les avantages*'.⁵⁰ But when Ampère replaced the linear currents in the apparatus of Fig. 1 with helices, he had no reason to include compensating axial currents. In this situation, since the radii of his helices were relatively small, Ampère realized that the interaction between the longitudinal components of the

48 See the quoted passage referenced by (43).

49 L. Pearce Williams has argued for a 25 September date but can cite no textual evidence. Why would Ampère say that his anomalous discovery suddenly delayed his plans to test his force law hypothesis if he made that discovery prior to the detection of electrodynamic forces between linear currents? As always, it is possible that Ampère simply confused the order of his own discoveries. For further discussion of this issue, see Williams and Blondel, *op. cit.* (5).

50 Ampère, *op. cit.* (41), p. 176.

spires of the two helices became predominant over the radial circular components and thus had produced an effect equivalent to an interaction between two linear currents.

This was the argument Ampère presented to the Académie on 6 November 1820. He realized that his reasoning hinged upon the legitimacy of treating each of the ‘portions infiniment petites’⁵¹ of an electric current as if it were made up of distinct components, and then determining what force would be contributed by each individual component treated in isolation from the others. The legitimacy of adding together forces imagined to be produced by various components of the elements of an electric current became a permanent principle of crucial importance for the mathematical expression of Ampère’s electrodynamics. He always referred to it as a *loi*, and I shall call it Ampère’s ‘addition law’.

Unfortunately, Ampère’s initial statements of this principle were far from clear. This was partially due to the fact that, by the time Ampère presented the law to the Académie on 6 November, his thoughts were already leaping ahead to possible applications. He naturally realized that he had to present experimental evidence in support of the addition law; simultaneously, however, he was already trying to incorporate the addition law into a derivation of the angular part of his force law. The experimental project called for a clear, concise statement of the law in question followed by experimental evidence of obvious confirmatory value. But Ampère’s impatience to use the law as a premise for derivational purposes infected his early terminology with the ambiguities he was struggling with on the more abstract level of mathematical idealization. Further study of Ampère’s addition law requires us to penetrate the smokescreen of assertive rhetoric about ‘facts’ and ‘laws’ that surrounded his gradual creation of a new conceptual framework. This calls for a brief digression in order to clarify the issues at stake.

Throughout the month prior to 6 November, Ampère had been grappling with the central theoretical concept of his electrodynamics, namely, the idea of a segment of an electric current that could be considered ‘infinitely small’ in comparison to measurable magnitudes but to which a three-dimensional direction could also be assigned. His explanation of anomalous helix behaviour had relied upon a further resolution of current elements into components; it is not surprising that Ampère initially had difficulty explaining the meaning of this operation. First of all, even ‘infinitely small’ circuit segments were assigned lengths in order to represent them geometrically. For Ampère, these lengths were a relative measure of the number of elementary molecular processes taking place within a current segment at any instant; these are the oscillations of the two electric fluids responsible for the electrodynamic forces contributed by the segment. Thus, as Ampère wrote in an 1820 draft,

... une portion infiniment petite exerce nécessairement une action proportionnelle à sa longueur, puisqu’en la subdivisant en un nombre quelconque de parties égales, son action est la somme des actions de toutes ces parties, lesquelles sont nécessairement égales entre elles ...⁵²

Hopefully, the idea Ampère tried to convey on 6 November was that the force in any direction due to an infinitely small current element was equivalent to a sum of individual

⁵¹ Ampère, op. cit. (41), p. 174.

⁵² Ampère, Archives of the Académie des Sciences, carton 8, chemise 158. For one of Ampère’s fullest published discussions of this issue, see Ampère, op. cit. (13), pp. 199 and 296–302.

forces in that direction when these forces are imagined to be produced by the components into which the original element has been resolved.⁵³ Drawn geometrically, the length of each component would represent the maximum strength of the force it could exert when parallel to another current element at a specified distance.

In addition, however, Ampère also wanted his forces to be proportional to the 'intensity' of electric currents. This quantity would represent the number of molecular processes taking place at any point in the circuit during a given unit of time; intensity thus *does not* correspond to the length of Ampère's geometric depictions of current elements. It is not clear that Ampère himself ever seriously misunderstood this distinction; his instinctive insight into the implications of his mathematical models seems to have been unimpaired by the ambiguous language that persisted even in the initial published version of the derivation he presented on 4 December.

What we can be sure of is that Ampère's early statements of his addition law so thoroughly confused current element 'length' with current 'intensity' that few of his readers could have made much sense of them. For example, in one of his earliest and most detailed published discussions of the law, Ampère wrote that in order to understand the law:

... il faut concevoir dans l'espace une ligne représentant en grandeur et en direction la résultante de deux forces qui sont semblablement représentées par deux autres lignes, et supposer, dans les directions de ces trois lignes, trois portions infiniment petites de courans électriques, dont les intensités soient proportionnelles à leurs longueurs. La loi dont il s'agit consiste en ce que la petite portion de courant électrique, dirigée suivant la résultante, exerce, dans quelque direction que se soit, sur un autre courant ou sur un aimant, une action attractive ou répulsive égale à celle qui résulterait, dans la même direction, de la réunion des deux portions de courans dirigées suivant les composantes.⁵⁴

If one is not misled by the preliminary comments about 'intensities', the statement of the addition law in the last sentence of this passage is fairly accurate. At the 6 November meeting of the Académie, Ampère tried to provide experimental evidence for his law using the helices that had suggested it. According to his recollections of this meeting, he gave a helix a compensating axial current and showed that the earlier anomalous attractions and repulsions now were cancelled out so that his apparatus remained stationary in a state of equilibrium. Ampère left no detailed record of how this demonstration was carried out. He may have used some combination of the apparatus shown in Figs 1 and 3, or he may have wrapped a helix around a vertical glass tube.⁵⁵ Our ignorance on this point is unfortunate because this may well have been Ampère's first encounter with the type of static situation that he would rely upon in his subsequent

53 To my knowledge, no immediate record was made of what Ampère said at the 6 November meeting of the Académie. For what are probably the earliest recorded recollections by Ampère himself, see Ampère, *op. cit.* (47), p. 168. A page of Ampère's notes for his presentation is in the Archives of the Académie des Sciences, carton 8, chemise 160.

54 Ampère, *op. cit.* (41), p. 174.

55 In Ampère's most detailed description of his demonstrations, he refers to an instrument of the type shown in Fig. 3; Ampère, *op. cit.* (47), pp. 168–169. For a few additional details, see Laumont, *op. cit.* (47), pp. 543–546; Ampère, *op. cit.* (41), pp. 175–176 and 208–209; and a set of notes for Ampère's 26 December 1820 lecture to the Académie, Archives of the Académie des Sciences, carton 8, chemise 164.

equilibrium experiments. Considering how many issues were on Ampère's mind at this time, it is not surprising that he did not immediately attribute any special significance to this particular observation. He apparently spent most of his lecture on 6 November using the apparatus of Fig. 3 to show how a compensated helix interacts as expected with bar magnets and compass needles.

At any rate, we can conclude that Ampère's initial demonstrations in support of the addition law were examples of the hypothetico-deductive mode of reasoning Ampère called *indirect synthesis*; effects produced using compensated helices confirmed the implications of the addition law and Ampère's theory of magnetism. But the state of equilibrium Ampère observed with one of these helices also set a precedent that he would build upon several weeks later.

Meanwhile, Ampère realized that he had to respond to the rival Laplacian interpretation of Oersted's discovery as presented to the Académie by Biot and Savart on 30 October. As part of this response, he used the addition law to present his first public derivation of part of his electrodynamic force law.

D. Ampère's first public discussion of his force law: 4 December 1820

Although by 1826 Ampère had at least some justification for claiming that he could derive his force law from a well-chosen set of equilibrium experiments, this certainly was not the case in 1820. On 4 December he presented a derivation of the *angular* factor in the law using symmetry arguments and his addition law. The full details of this derivation are not relevant to my concern to clarify how investigations of the addition law stimulated Ampère's gradual invention and application of the equilibrium technique. At this point I shall simply summarize the general structure and presuppositions of Ampère's argument and explain how it relied upon the addition law. This will establish the context for Ampère's attempts to improve the experimental basis for the addition law and justify his mathematical manipulations of idealized current elements. As will soon become apparent, Ampère's 'derivation' was an artful combination of idealization, mathematics and approximation. His invention of the equilibrium experiment technique during the following eighteen months was directly inspired by his extended efforts to legitimate controversial stages in his argument.

I have already mentioned that Ampère's 4 December derivation concerned only the angular factor in his formula. At this time he simply continued to assume that the force would include a separable inverse square function of the distance between two current elements.⁵⁶ Secondly, he also assumed that the force would act along the line joining the elements and that, when the direction of one of the two elements is reversed, the mutual force also reverses its direction but retains its original magnitude. He used the last of these assumptions to argue for a useful symmetry principle: the electrodynamic force between two current elements should vanish when one element is located in a plane, that

⁵⁶ Ampère, 'Note sur un Mémoire lu à l'Académie royale des Sciences, dans la séance du 4 décembre 1820', *Journal de Physique* (1820), 91, pp. 226–230.

passes through the centre of the other element and cuts it at a right angle.⁵⁷ Like the addition law, this symmetry principle applies to the idealized geometrical representation of *infiniment petites* portions of a circuit depicted for mathematical purposes as tiny directed line segments.

At the Académie on 4 December, Ampère presented a derivation of the angular factor in his force law using the above symmetry principle and his addition law. He began by giving two current elements a mutual orientation determined by the three angles α , β and γ , as shown in Fig. 2. He then resolved each element into components along three conveniently chosen directions and considered the forces between all possible pairs of components when one component is chosen from each of the two current elements. Geometrically, the components of a current element have no common point of intersection; they only combine to form the total element through a process that now would be called vector addition. For the purposes of his derivation, however, Ampère imagined the components of each element to be slightly transported so that all three of an element's components could be depicted as having their midpoints at the point originally chosen as the midpoint of the element itself. This was justified by the relatively small distances involved in comparison to the distance between the two current elements.

With his components restructured in this way, Ampère could apply his symmetry principle to conclude that there were no forces between some pairs of components; he then invoked the addition law to add together the remaining forces. The resulting total force between two current elements is given by the following formula where g and h are the current intensities, k is an undetermined parameter, and r^{-n} is the distance dependency which Ampère assumed to be such that $n = 2$,

$$g \cdot h \cdot r^{-n} (\sin \alpha \sin \beta \cos \gamma + k \cos \alpha \cos \beta). \quad (2)$$

This expression was more complicated than the hypothesis Ampère had tentatively adopted in October; it included an additional term weighted by the magnitude of the parameter k which was left undetermined by the derivation Ampère presented on 4 December. In physical terms, k represented the relative strength of the force between two collinear current elements in comparison to the force that would exist if they were parallel and separated by the same distance.⁵⁸

During the remainder of December, Ampère argued on the basis of some limited experimental evidence that k was small enough to be treated as vanishing in comparison to measurable quantities. This reduced his force law to the form of his initial hypothesis, and Ampère continued to believe that this truncated formula was correct until early in 1822. At that point an anomalous observation instigated a renewed scrutiny of the more general formula (2) resulting in the discovery that $k = -1/2$.⁵⁹

57 Ibid., pp. 226–227. A more detailed argument is provided in a manuscript draft for Ampère's memoir: Archives of the Académie des Sciences, carton 8, chemise 162.

58 Ibid., p. 229. Ampère adopted the notation of n and k for his two parameters during 1821. He originally simply used r^{-2} for the assumed distance dependency and wrote the parameter k as a ratio n/m to express the fact that it determines the relative strength of two forces.

59 This point is discussed in detail in J.R. Hofmann, 'The great turning point in André-Marie Ampère's research in electrodynamics: a truly "crucial" experiment', 1982 Ph.D. dissertation, University of Pittsburgh, University Microfilms order no. 8318183.

However, in December 1820 these later developments were far in the future. At this point it was imperative that Ampère devote attention to the experimental basis for the addition law; this was the problem that soon fostered his production of new equilibrium phenomena.

E. Ampère's first equilibrium apparatus: December 1820–January 1821

During the month following the 4 December meeting of the Académie, Ampère struggled with two closely related projects. First, because Biot and Savart were promoting their own Laplacian programme in electrodynamics, Ampère devoted considerable energy to what turned out to be unsuccessful attempts to design a decisive experimental test that would discredit their approach in favour of his own.⁶⁰ Secondly, when these efforts resulted in an acknowledgment that the observable implications of the two programmes were presently indistinguishable, this amplified Ampère's realization that he needed to call upon a new mode of argumentation if his own conception of the *fait primitif* of the electrodynamic domain was to be accepted as the correct one.

In particular, Ampère had to motivate the acceptance of his addition law, a crucial premise in his derivation of the angular factor in the electrodynamic force law. In view of Ampère's predisposition for *direct analysis*, it is not surprising that he thought of improving the design of one of the experiments he had used in his initial presentation of the addition law to the Académie on 6 November. This had been his demonstration that an axially compensated helix does not exert any of the forces to be expected from a linear current. Although Ampère presumably performed or at least described such an experiment for the Académie on 6 November, the fact that he never published an accurate description of the original experiment is indicative of the chaotic state of affairs he sought to remedy during the following December.

Ampère's project was determined by the mathematical technique he had used in his 4 December derivation. How could he provide a readily repeatable demonstration that could be interpreted as a legitimization for a mathematical analysis of the force attributed to a current element as if this force was a sum of forces produced by geometrically depicted components of the original element? This was the problem that soon generated Ampère's interest in equilibrium phenomena. Clearly, there would always be a gap between physically manipulated circuitry and the idealized geometry of *infinitement petites* current elements. Ampère's task was to make this gap relatively easy to cross by means of an experimentally stabilized stepping-stone.

Although we do not know how Ampère went about presenting his ideas verbally, his publications and notes leave a fairly clear record of his strategy. First, the disastrous confusion of current element length and current intensity was purged from subsequent statements of the addition law. For example, late in 1820, when Gillet de Laumont composed an exposition of Ampère's research, he relied heavily upon passages written by Ampère for his own publications. In the description of the 6 November presentation of

60 The primary sources for these arguments are the unpublished manuscripts pertaining to Ampère's lectures to the Académie during December 1820 and January 1821: Archives of the Académie des Sciences, carton 8, chemises 162, 163 and 166. They are discussed in Hofmann, op. cit. (59).

the addition law to the Académie, Laumont replaced Ampère's original phrase, 'portions infiniment petites de courans électriques, dont les intensités soient proportionnelles à leurs longueurs',⁶¹ by a much more accurate reference to, 'courans électriques dont les forces attractives ou répulsives sont proportionnelles à leurs longueurs'.⁶²

This certainly was a more correct statement of the significance Ampère attributed to the lengths of his geometric representations of current elements. But in addition to this conceptual clarification, Ampère decided to formulate an experimentally testable assertion that he hoped would readily suggest the necessary transition from experience to his idealized mathematical model. In passages published early in 1821, he claimed, somewhat misleadingly, that the addition law 'se réduit à' the following statement:

Si l'on fixe la direction d'un courant électrique deux points infiniment rapprochés, et qu'on substitue à la petite portion de courant comprise entre ces points une autre portion de ce même courant, suivant une ligne pliée ou contournée d'une manière quelconque, mais se terminant aux mêmes points sans s'en écarter nulle part à une distance finie, cette substitution ne changera en aucune manière l'action exercée dans quelque direction que ce soit par la petite portion de courant que l'on considère, sur une autre portion de courant électrique éloignée de la première d'une quantité finie.⁶³

This assertion was in fact a testable claim about wire segments large enough to be manipulated experimentally. The phrase 'infiniment rapprochés' can simply be paraphrased as 'very close together in comparison to other lengths or dimensions of the circuit'. The addition law itself actually applied to the idealized geometry of current elements and could not be argued for directly; Ampère thus was hoping that it would be accepted as an obvious 'deduction' if he could provide a striking demonstration in support of its experimentally testable analogue.

Ampère made this project public at the 26 December meeting of the Académie. According to the surviving manuscript notes for Ampère's presentation, he stated the testable assertion as quoted above and then described the apparatus he had designed, 'pour m'assurer directement de l'exactitude de cette loi'.⁶⁴ Ampère's own sketch is shown in Fig. 4. EG and FH represent two glass tubes equidistant from a central point N which acts as a point of suspension for either a mobile linear circuit segment or, as shown in Fig. 4, a small magnet. Either linear or slightly bent conductors are to be inserted in grooves inside the glass tubes. Ampère's description of the experiments he planned to perform is worth quoting because this is probably the message he conveyed to the Académie on 26 December.

La verification de la loi dont je viens de parler consiste à s'assurer que la substitution d'un de ces conducteurs à l'autre dans une meme rainure ne change rien à l'action exercée par exemple sur

61 Ampère, op. cit. (41), p. 174.

62 Laumont, op. cit. (47), p. 544.

63 Ampère, 'Note sur deux mémoires lus par M. Ampère à l'Académie royale des Sciences, le premier dans la séance du 26 décembre 1820; le second dans les séances des 8 et 15 janvier 1821', *Journal de Physique*, (1821), 92, pp. 160–165; on p. 161. The same passage appears in Ampère, 'Exposition du moyen par lequel il est facile de s'assurer directement, et par des expériences précises, de l'exactitude de la loi des attractions et répulsions des courans électriques, suivie de quelques observations sur cette loi. Mémoire lu le 26 décembre 1820', *Annales des Mines*, (1820), 5, pp. 553–558; on p. 554.

64 Ampère, Archives of the Académie des Sciences, carton 8, chemise 164.

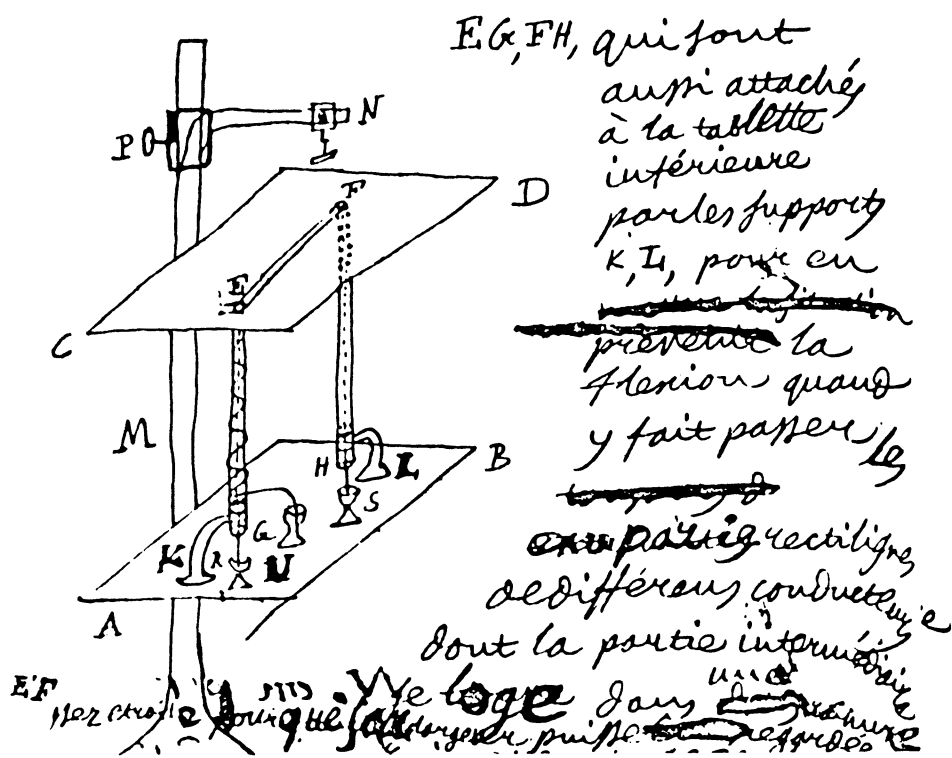


Figure 4. Ampère's drawing for the apparatus described for the Académie on 26 December 1820.

un petit aimant suspendu en N. On peut faire une sortie de contre épreuve de cette expérience en plaçant deux courants dirigés en sens contraire dans la même rainure séparés par une lame de papier, en se servant pour cela d'un même fil conducteur dont une extrémité arriverait par le tube EG, par exemple, et l'autre reviendrait en l'entourant de quelques spires se rendre dans la coupe à mercure U située près de la coupe R, et qui remplacerait alors l'autre coupe S. Il n'y aurait aucune action sensible.

Cette dernière expérience ne serait, au reste, que celle que j'ai faite sur un conducteur rectiligne enfermé dans un tube de verre, et qui revenait ensuite autour de ce tube en l'entourant de ses spires, expérience dont j'ai déduit la loi dont il est ici question, avec seule différence qu'au lieu d'un fil conducteur plié en hélice on peut employer un qui soit contourné d'une manière quelconque.

Dès que cet instrument sera achevé, je ferai les expériences auquel il est destiné.⁶⁵

There are several points to be noted here. Although Ampère planned his circuit to pass through both of whatever conductors were placed within the two glass tubes, he did not originally attempt to demonstrate a state of equilibrium for the suspended magnet or conductor. Instead, expecting to always observe some rotational reaction, he only claimed to be able to duplicate a linear conductor's contribution to any observed effect by replacing the linear wire with a bent and twisted conductor. Almost as an after-

65 Ibid.

thought, Ampère also described 'a kind of counter-test of this experiment' in which only one tube is used, but with a linear conductor inside and an arbitrarily twisted conductor coiled on the outside. This arrangement was expected to result in the suspended part of the apparatus remaining unaffected and in a state of equilibrium. Ampère clearly did not consider this to be a major improvement over his 6 November helix experiment, and he did not yet place any particular importance on the use of equilibrium experiments.

But on 26 December, Ampère had not yet constructed his apparatus; he apparently had some unexpected experiences as he grappled with it during the following two months. To my knowledge, Ampère left no records of his initial observations and reactions. All that is certain is that by the time he wrote up a slightly retrospective memoir for the February 1821 volume of the *Journal de Physique*, Ampère was ready to launch into an elaborate description of the famous apparatus he would later depict in engravings such as that shown in Fig. 5.⁶⁶

In contrast to the tentative circuits described on 26 December, this new apparatus was specifically designed to produce a state of equilibrium; the resulting demonstration thus became the first and prototypical member of the set of equilibrium experiments from which Ampère eventually would claim to derive his entire force law. As shown in Fig. 5, the circuit includes a vertical suspended linear segment, GH, equidistant from two longer conductors. One of these is a linear segment, QP, and the other is an arbitrarily twisted conductor, SR, which follows a random, two dimensional path of tiny bends. The suspended conductor, GH, is free to rotate about a vertical axis, FI, and the circuit is designed so that the linear and twisted conductors exert oppositely-directed repulsive forces on GH. Ampère accomplished this by making use of an axially-compensated helix, gf, a technique that creatively embedded the humble origins of his apparatus within its own final structure.

According to Ampère's assessment of this circuit, if the linear and bent conductors act with unequal strengths:

... le conducteur mobile seroit dévié par une force égale à la différence de ces deux actions, au lieu que si la loi énoncée plus haut est exacte, ce conducteur doit rester dans la situation où on l'avoit mis avant d'établir les communications, en équilibre entre deux forces égales. C'est en constatant qu'il en est en effet ainsi, que l'expérience démontre l'exactitude de cette loi.⁶⁷

The 'law' Ampère refers to as demonstrated here is the analogue to the addition law phrased in terms of 'small portions' of wire which are actually large enough to be individually manipulated. His argument that this assertion is demonstrated by the observed state of equilibrium hinges upon the fact that an arbitrary non-systematic set of

66 Ampère, op. cit. (63), pp. 161–163. To my knowledge, the engraving depicted in Fig. 5 was used for the first time in Ampère's republication of his *Annales des Mines* article; Ampère, op. cit. (63). This reprint appeared in Ampère's collection of memoirs which in its second edition was given the title, *Recueil d'Observations Électro-dynamiques Contenant Divers Mémoires, Notices, Extraits de Lettres ou d'Ouvrages Périodiques sur les Sciences, Relatifs à l'Action Mutuelle de Deux Courants Électriques, à Celle qui Existe entre un Courant Électrique et un Aimant ou le Globe Terrestre, et à Celle de Deux Aimans l'Un sur l'Autre*, 2nd edn., Paris, 1822, pp. 87–92. For details on the two editions of this collection, see Williams, op. cit. (5), 1983, p. 494.

67 Ampère, op. cit. (63), 1821, p. 162.

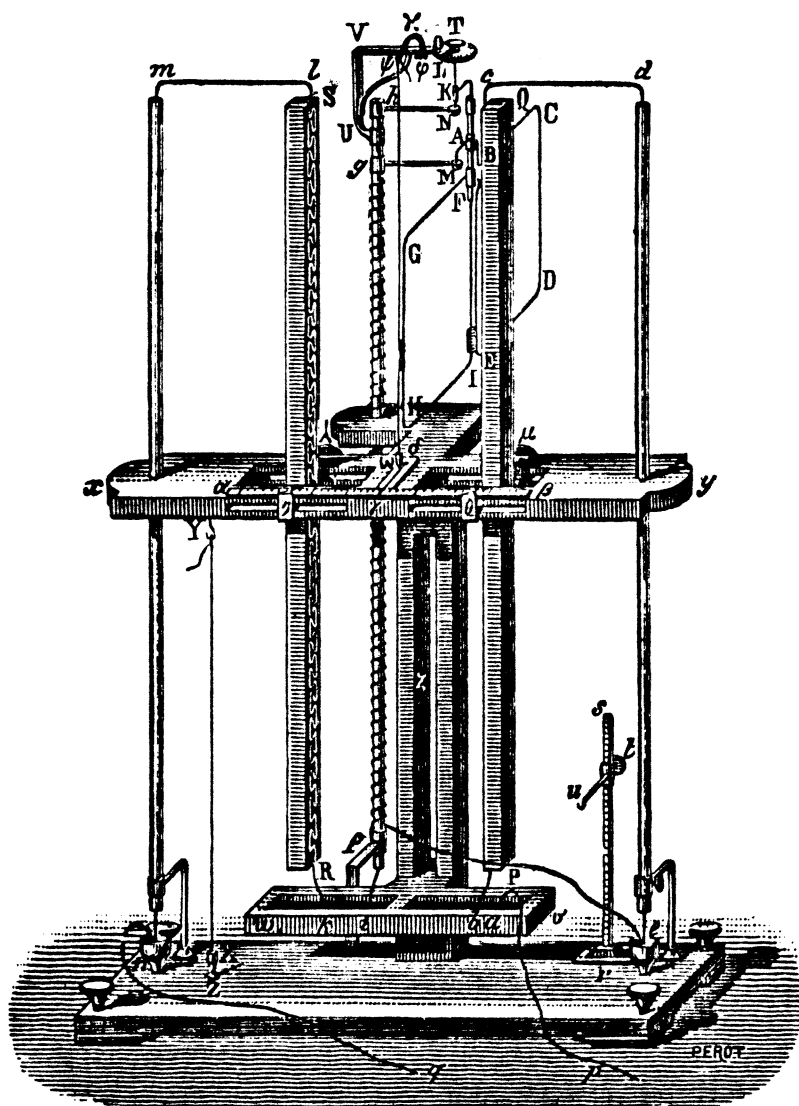


Figure 5. The final design of Ampère's first equilibrium apparatus.

bends had been given to the twisted conductor, SR. That is, any specific little bend in SR could have been slightly different than it actually was without preventing the resulting state of equilibrium. For Ampère, this meant that, although what was observed was a state of equilibrium produced by the combined action of the entire linear and twisted conductors, this observation justified the 'law' he had asserted for any individual small portion of the circuit.

Furthermore, as far as Ampère was concerned, the addition law for individually unobservable current elements could be 'deduced' straightforwardly from the preceding

preliminary conclusion. Ampère never published a detailed discussion of this step in his argument; he seems to have expected that, once his readers had accepted the conclusion he had already argued for, they would naturally extend it by imagining the circuit segments in his apparatus to become increasingly small. Ampère himself was quite willing to pursue this reasoning until it applied to current elements so small as to be mathematical idealizations that could only be depicted and manipulated geometrically.

Nevertheless, Ampère also realized that some of his colleagues were reticent about following his lead in this matter. To ensure that they at least accepted the validity of his experimental discoveries, Ampère published a retrospective expository memoir early in 1821. In the process of summarizing his research during 1820, Ampère drew a careful distinction between the experimental 'fact' produced by his equilibrium apparatus, and 'les lois mathématiques des attractions et répulsions de deux fils métalliques faisant partie d'un circuit voltaïque'.⁶⁸ Among these laws he included the following accurate statement of the addition law as he had used it in practice:

Que si l'on considère une de ces portions infiniment petites comme la diagonale d'un parallélepède, son action sur l'autre est égale à la somme des actions qu'exerceroient sur cette dernière, trois portions du premier fil conducteur dirigées suivant les trois arêtes qui mesurent les trois dimensions du parallélepède, et de même longueur que ces arêtes.⁶⁹

Ampère rather optimistically presumed that the addition law would be accepted as a straightforward implication of his equilibrium demonstration together with his interpretation of the import of that demonstration for the individual small circuit segments of his apparatus. This expectation continued during the composition of the *Exposé* Ampère and Babinet completed by July 1821. Once again, the addition law was said to be legitimated by imagining an infinitesimal current element to be replaced by three components, 'qui d'après l'expérience fait le même effet'.⁷⁰

Ampère's strategy is not too surprising considering the Laplacian milieu in which he was arguing. Biot too had formulated his own conception of the electrodynamic *fait primitif* in terms of an infinitesimal current element acting on a particle of magnetic fluid. In contrast to Ampère, however, Biot treated his current elements as if they were punctal; he did not develop a theory dependent upon their orientation. The importance of Ampère's first equilibrium demonstration thus was due to the support it provided for his efforts to initiate an alternative research programme built around a force law for directed current elements. The success of this programme depended largely upon the degree to which it achieved an accurate and comprehensive reductionistic explanation of electrodynamic phenomena. My concern in this essay is to explain how Ampère deliberately produced some of these phenomena in retaliation to what seemed to be serious experimental anomalies. Furthermore, by incorporating compensated helices into the circuitry of his first equilibrium apparatus, Ampère made the engraving shown in Fig. 5 a visual display of the conceptual and experimental route he had followed.

68 Ampère, 'Exposé sommaire des divers mémoires lus par Mr. Ampère à l'Académie Royale des Sciences de Paris, sur l'action mutuelle de deux courans électriques, et sur celle qui existe entre un courant électrique et le globe terrestre ou un aimant', *Bibliothèque Universelle des Sciences, Belles-Lettres et Arts*, (1821), 16, pp. 309–319; see pp. 311 and 318.

69 Ibid., p. 318.

70 Ampère and Babinet, op. cit. (39), p. 44.

CONCLUSION

Ampère's invention of the equilibrium experiment technique obviously did not come about in a single flash of inspiration. It is true that his longstanding preference for the *direct analysis* of simple theoretical assertions from more complex experimental observations did give him a predisposition to pursue this procedure in the electrodynamic domain; this distinguished Ampère from his more inductively minded Laplacian colleagues such as Biot. Nevertheless, Ampère's first systematic use of equilibrium apparatus only came about as an unexpected result of a gradual refinement of the demonstrations he had performed with axially compensated helices during the first week in November 1820. These initial demonstrations were designed in response to the anomalous behaviour of the uncompensated helices he had hoped would duplicate the interaction of two bar magnets. Ampère now attributed these effects to the longitudinal components of his helix spires. More fundamentally, he argued that all electrodynamic attractions and repulsions could be understood as cumulative effects due to the summation of forces attributed to appropriate components of individually unobservable current elements.

Following his application of this addition law to derive the angular factor in his force law, Ampère temporarily shifted his attention to a more directly testable version of the law stated in terms of circuit segments large enough to be individually observable in experimental apparatus. By 26 December, he had designed equipment to produce phenomena he hoped would compel acceptance of this relatively large-scale analogue to the addition law. Most of the initial demonstrations he planned at this time were not intended to establish states of equilibrium; only his subsequent experience with his apparatus revealed that such effects not only were technologically feasible but might even be particularly appropriate. Early in 1821, this investigation resulted in the invention of the famous bent wire equilibrium apparatus depicted in Fig. 5.

At this point, Ampère's argumentation passed through a sequence of construals and interpretations.⁷¹ He began by giving his new equilibrium effect extensive publicity by means of detailed descriptions of his apparatus. He was particularly conscientious about this due to the less than enthusiastic response generated by the haphazard juxtaposition of experiment and theory he had presented in his most widely read 1820 memoir in the *Annales de Chimie et de Physique*. His consternation about this subject is evident in a March 1821 letter to Bredin.

Quelque longue que fût une lettre que je t'écrirais sur tout cela, elle ne serait pas plus claire que mon mémoire. Personne ne le comprend? Je suis sûr que ce qu'ils cherchent à comprendre c'est

71 For an interesting discussion of 'construals' as 'flexible, quasi-linguistic messengers between the perceptual and the conceptual', see D. Gooding, 'How do scientists reach agreement about novel observations?', *Studies in History and Philosophy of Science*, (1986), 17, pp. 205–230. Gooding's examples are drawn from the electrodynamics of Ampère's English contemporaries: Davy, Faraday and Barlow. This group rejected Ampère's reduction of electrodynamics to central forces. Engravings such as those shown in Figs 1 and 5 are examples of Ampère's efforts to highlight the phenomena he held to be of primary importance for a correct understanding of electrodynamics. As such, his diagrams and descriptions were suggestive construals of electrodynamic phenomena which Ampère then subjected to highly theoretical interpretation.

la théorie dont je ne voudrais pas qu'on s'occupât encore, on l'adoptera de reste quand les faits seront bien connus. Ce sont les faits, les faits qu'il s'agit de bien connaître.

Est-ce que les physiciens de Lyon ne peuvent pas distinguer dans mon mémoire, malgré le peu d'ordre qui y règne parce que je l'écrivais à mesure de nouvelles expériences, les faits nouveaux des théories?⁷²

Ampère's optimism that his version of electrodynamics would win acceptance once the 'facts' were known was not so heady as to prevent him from providing some explanatory encouragement. He gave brief but pointed arguments for how his equilibrium apparatus produced effects that could be interpreted as an experimental basis for the large-scale analogue to the addition law when stated in terms of the individually observable segments of his circuitry. This is an outstanding example of Ampère's use of what he called *direct analysis*. He was less helpful, and less successful, in promoting the acceptance of the addition law itself. In fact, Ampère's attribution of forces to components of 'infinitesimal' current elements initially generated some highly critical reactions. His reliance upon a carefully designed state of equilibrium clearly did not result in a sudden victory over the rival Laplacian programme headed by Biot.

For example, as late as 5 July 1822, Ampère's close friend Maurice reported the following objections from a correspondent who wished to remain anonymous:

Ce n'est pas tant la contention d'esprit nécessaire pour saisir les actions des courants qui me répugne dans cette théorie que le grand nombre d'hypothèses toutes gratuites l'abus de la considération des infiniment petits avec lesquels on peut dire tout ce qu'on veut, et le mélange de certaines idées dynamiques dont l'introduction n'est pas suffisamment motivée ni l'influence nettement caractérisée.⁷³

Ampère's attempt to motivate his conceptual and mathematical treatment of 'infinitesimal' current elements obviously had failed to convince this critic.

The status of Ampère's research programme was further undermined by Biot's promotion of a rival programme publicized by a great display of systematically collected quantitative data. Although Biot and Savart were at a disadvantage due to their reliance upon a non-central, transverse force between a current element and a particle of magnetic fluid, Biot claimed that this was only a temporary expedient that would become unnecessary when the 'impressed magnetism' of conductors was understood in terms of central forces between magnetic fluid particles. This project never materialized, but it was still promoted enthusiastically by Biot during 1821.⁷⁴

Like Biot, Ampère claimed that he too would eventually reduce his force law to more elementary forces; in Ampère's case these forces were assumed to act between the electric fluid particles of the ether. But Ampère made no more progress on this reduction than Biot did on his. As a result, during most of 1821 the competition between Biot and Ampère continued to call for a pragmatic choice between two force laws which were admitted to be temporary expedients. In this contest Ampère failed to generate wide acceptance of his construal of electrodynamic phenomena. Ampère's reliance upon the

⁷² Ampère, *Correspondance*, vol. 3, p. 907.

⁷³ *Ibid.*, p. 924.

⁷⁴ Biot, *op. cit.* (21).

addition law met considerable resistance due to the delicate reasoning through which he claimed to ‘deduce’ it from his equilibrium demonstration.

Furthermore, in 1821 Ampère had no expectations that the equilibrium method might provide a basis for further derivations relevant to his force law. During 1820 and 1821, his attempts to stipulate the values of the parameters k and n in his force formula (2) were simply hypothetico-deductive confirmations of consequences drawn from his assumptions that $k = 0$ and $n = 2$. This was the method Ampère called *indirect synthesis*, and prior to April 1822 he had no hopes of being able to do without it. At that point, confrontation with another anomalous observation inspired Ampère’s invention of a second equilibrium experiment. His analysis of this demonstration provided the relationship $2k = 1 - n$, and this sparked Ampère’s hopes that the entire derivation of his force law might eventually be based upon an analysis of states of equilibrium.⁷⁵

By 1826, a full set of four equilibrium experiments took a central role in Ampère’s most polished attempt to improve the reception of his ideas. His first equilibrium demonstration thus set an unexpectedly important precedent for Ampère’s subsequent procedure. Furthermore, the degree to which other physicists considered the conceptual framework of Ampère’s electrodynamics to be well-founded was to a large extent dependent upon their willingness to follow him in his reductionistic interpretation of carefully constructed cases of equilibrium as a route to understanding the microstructure of electrodynamic events.

APPENDIX

Major events during the early months of Ampère’s research on his electrodynamic force law

1820

- | | |
|---------------------------------|---|
| 25 September | Ampère demonstrates for the Académie that conducting planar spirals attract and repel each other and respond to bar magnets like magnetic poles. Arago follows Ampère’s suggestion and magnetizes a needle by wrapping it with a helical conducting wire. |
| 2 October | Ampère presents the Académie with the first part of his résumé article to be published in volume 15 of the <i>Ann. Chim. Phys.</i> ; by this time Ampère had discovered electrodynamic forces between linear conducting wires. |
| 9 October | At the Académie, Ampère demonstrates electrodynamic forces between linear conducting wires (Fig. 1). |
| Late September to early October | Ampère uses a centrally suspended conducting helix wrapped around an axial current to duplicate the rotational response of a suspended bar magnet to another bar magnet (Fig. 3). |

⁷⁵ This transition in Ampère’s thinking is the primary topic of Hofmann, op. cit. (59).

17 October	Ampère shows Biot and Gay-Lussac the new apparatus with which he discovered the action of terrestrial magnetism on a suspended current loop.
During the month after 25 September	Probably shortly after 17 October, Ampère gets anomalous results when he substitutes two helical conductors without axial currents for the linear conductors in the apparatus he presented to the Académie on 9 October (Fig. 1).
30 October	At the Académie, Ampère demonstrates the action of terrestrial magnetism on a suspended current loop. Biot and Savart present the Académie with their first report on their quantitative measurements of Oersted's discovery.
6 November	At the Académie, Ampère presents his addition law for electrodynamic forces and uses it to interpret the action of helices. He uses a helix around an axial current to duplicate the action of a bar magnet on another bar magnet (Fig. 3). He mentions that the motion of ether particles might be the basis for the addition law.
4 December	At the Académie, Ampère presents his symmetry principle and uses it and the addition law to derive the angular factor in his electrodynamic force law. Biot gives a preliminary statement of the result of his recent measurements with Savart.
11 December	At the Académie, Ampère reports on an experiment which he claims implies that $k = 0$; he explains why an experiment by Gay-Lussac and Thenard erroneously seemed to indicate that k had a positive value. Assuming that $k = 0$, he draws an analogy to the radiation of heat. He reports an inconclusive attempt to use quantitative measurements to demonstrate the superiority of his theory to Biot's.
18 December	Biot and Savart report to the Académie on their second set of measurements on a variation of Oersted's discovery.
26 December	At the Académie, Ampère describes an apparatus with a suspended magnet to test his addition law (Fig. 4). He presents the second part of his résumé memoir to the Académie for publication in volume 15 of the <i>Ann. Chim. Phys.</i>
Late December or early January 1821	Ampère designs an equilibrium apparatus with a suspended linear conducting wire to demonstrate his addition law (Fig. 5).